In the beginning of the year 1875 I removed to South Wales, and soon discovered that all the collieries in which great explosions had occurred were dry and dusty. My previous experience was almost wholly confined to damp mines, in all of which, with one exception (Nitshill), there was no coal-dust, and in none of which, with the same exception, a great explosion had ever occurred. The impression referred to above probably exercised a certain influence at that time in helping me to form the conception of what I believe to be the true explanation of great colliery explosions. In the words of my first paper[1] it is as follows: "If it could be shown that a mixture of air and coal-dust is inflammable at ordinary pressure and temperature, there would be no difficulty in accounting for the extent and violence of many explosions which have occurred in mines in which no large accumulations of fire-damp were known to exist; for it is only necessary to suppose that a violent gust of wind (originated, for example, by the explosion of a small accumulation of fire-damp) had swept through the adjoining galleries, raising a cloud of dust into the air, and then all the other phenomena would follow in regular order. The flame of the originally inflammable mixture would pass into the newly-formed one, expanding its volume; the disturbance would be propagated over an ever-widening area, until that area might possibly become co-extensive with the workings themselves; and *the consequences would be the same as if the whole space had been filled with an inflammable mixture before the disturbance began.*"

I demonstrated by an experiment first made on the 3rd of July, 1875, that air containing less fire-damp than can be detected by the ordinary means of testing the air in mines, is rendered inflammable at ordinary pressure and temperature when fine dry coal-dust is added to it. This was the first step towards proving the truth of my theory.

In the same paper I also stated, "It is always possible that if coal-dust could be made fine enough and were thoroughly mixed with dry air in the proportion of about 1 pound to 160 cubic feet of air, the mixture might be inflammable at ordinary temperature, or, *if not*, it might at least be so nearly inflammable that an explosion begun in it, in a confined space, might be propagated through it."

In my second (1879) and subsequent papers to the Royal Society, I stated the further opinion, first, as the result of more elaborate experiments, and secondly, as the result of a careful personal investigation and consideration of all the circumstances attending the occurrence of several great explosions, that, "A fire-damp explosion occurring in a dry coal-mine is liable to be indefinitely extended by the mixture of air and coal-dust produced by the disturbance which it initiates." This was the final step.

Of this, Sir Frederick says:[2] "Mr. Galloway was certainly the first to enunciate the conclusion that a small proportion of fire-damp is essential to impart to a mixture of air and coal-dust the power of propagating flame, though *he afterwards abandoned this conclusion in favour of the one some time previously accepted by Marreco*, to the effect that fire-damp is altogether unnecessary for the conveyance of flame with explosive effect by a mixture of dry coal-dust and air."

In January, 1878, I commenced a series of articles in *Iron* on coal-dust explosions, hoping thereby to arouse an interest in the subject, and, amongst other items of information, I gave translations of Vital's and Desbief and Chansselle's papers. Seven of these articles had already appeared when, in April, 1878, Prof. Marreco (to whom I had myself sent copies of them) and Mr. Morison, read their first paper before the Chesterfield Institute. They say, "Before the writers had the opportunity of learning what had already been accomplished abroad, their attention was directed to the subject by a paper read by Mr. W. Galloway before the Royal Society, in which he related his experiment in producing explosion in a mixture of fire-damp and air impregnated with dust. The opinion expressed in that paper, viz., that coal-dust was explosive only in an atmosphere containing a minute proportion of fire-damp, induced the writers to still further extend the scope of the experiments, and endeavour to discover whether coal-dust could by any means be exploded in an atmosphere totally free from fire-damp."

The arrangement to carry out the experiments here spoken of was made between Marreco and myself in the end of 1875 or beginning of 1876, and was referred to in my first paper (1876) thus: "It would be premature to draw any positive inference from my experiments with gunpowder shots, as they are by no means so satisfactory as I could wish, and I am glad to be able to state that Prof. Marreco, of Newcastle-on-Tyne, intends to investigate this question with more substantial apparatus."

Returning to Sir Frederick's remark, I would say that I abandoned no previous conclusion "in favour of one some time previously accepted by Marreco." I then held, and still hold, what is now a universally accepted opinion amongst mining men, that a mixture of air and fire-damp, which is not inflammable at ordinary pressure and temperature, may be rendered inflammable by the addition of dry coal-dust, and I was prepared, as soon as proof satisfactory to myself was forthcoming, to extend the area of my opinion, so as to include a mixture of fine dry coal-dust and air in the same category. This latter proof alone was necessary to the firm establishment of the theory enunciated at the commencement of my first paper, and for that reason Sir Frederick's statement appears to me to be wanting in logical sequence.

Neither the statement made by Marreco and Morison, that they intended to "endeavour to discover whether coal-dust could by any means be exploded in an atmosphere totally free from fire-damp," nor the results of the experiments published by them, appear to me to warrant Sir Frederick in taking up this position; for, concerning these very experiments of Prof. Marreco's he had himself remarked in the year 1880:[1] "It does *not appear that* in the numerous experiments made at Harton Colliery by the exposure of naked flames in currents of air laden with coal-dust, and by firing small cannon (representing blown-out shots) placed in various positions in such air-currents, *any indication has been obtained of a propagation of flame by the coal-dust.*" How then could Marreco have concluded that "fire-damp is altogether unnecessary for the conveyance of flame with explosive effect in a mixture of coal-dust and air"?

Sir Frederick continues to say, regarding myself: "Even his latest publication on the subject tends in the same direction, and emphasises his desire to 'claim that no earlier author had gone the length of crediting coal-dust with the *rôle* of principal agent, and relegating fire-damp to a secondary position.' The more recent results of other writers in this direction have, however, conclusively demonstrated that this is far too great a length to go, and that while on the one hand a very fine dry and highly inflammable coal-dust may, when raised and mixed with air by the force of a blown-out shot, become inflamed, and may then carry flame to considerable distances with a rapidity and violence of action similar to that of a fire-damp explosion, the extent to which, on the other hand, flame is propagated under corresponding conditions by most descriptions of coal-dust in the complete absence of fire-damp is very limited."

Sir Frederick and those whose opinions he here quotes appear to have omitted to take one element into consideration which has an important bearing upon the case. To this I may have occasion to refer more particularly at an early date.

W. GALLOWAY

Sunset-Glows

I WISH to call the attention of observers to a peculiar phenomenon which has been frequently noticed by me lately in connection with these sky-scenes. A very bright after-glow was visible here on October 27 last. I believe the date is correct, although I have unfortunately mislaid my day-book; the facts, however, I can vouch for otherwise, as they were detailed at the time. At 5 p.m. a heavy bank of cumulus was to be seen extending along the south-western sky-line, about 5° above and closely parallel to the horizon. High over this bluish bank of cloud rose the yellow haze of departing sunlight. This diffused after-glow was plainly intersected by numerous (I counted twenty-two) delicate streaks of nebulous stratus. These intersecting lines (they were scarcely "bands") were horizontal and parallel, piled up, as it were, above one another as high as 25° from the sea-line. The lowest hung apparently about half a degree above the gilded upper edge of the dark cumulus. On November 4, just before sunset, I exposed a gelatine plate, and succeeded in obtaining a photograph, in which seven of these narrow horizontal cloud-streaks can be faintly seen. On December 1 the following note was made:—4.20 p.m. Wind light,

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[1] *Proc. Roy. Soc.*, 1876.

[2] *Journal Soc. Arts* No. 20, 1885, p. 23.

[1] "Report on the Results of Experiments made with Samples of Dust collected at Seaham Colliery, &c.," 1880.

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N.N.W. Sunset-glow intersected by five horizontal dark cloud-streaks in the west. On the 2nd inst., at 4.20 p.m., five cumulo-strati were visible in the west, separated by parallel and horizontal orange-coloured bands. Above these were numerous (probably twenty) delicate dark lines traversing—also horizontally—the upper roseate after-glow. On the 10th and 11th inst., somewhat similar phenomena were visible here. On the 15th inst., with a light southerly air, eleven cloud-bands were seen by me at 4.30 p.m. In this case only, they were not parallel to the sea-line, but followed the direction of the west-north-west horizon. I have seen the same appearances once or twice since the last date. The above phenomena are new to me, and I have not met with any detailed account of them elsewhere. I have therefore ventured to address you on the subject. Can these cloud-streaks represent stratified air-dust in the upper regions of our atmosphere? W. AINSLIE HOLLIS

Hove, Brighton, December 26

Iridescent Clouds

THERE was a very striking display of iridescent clouds this afternoon. I noticed it first at about 3.40 p.m. The prismatic colours were pretty strongly marked, and the intense pearly brilliance of the delicate cirri was most striking. It is still visible (4.40 p.m.), though, of course, its lustre is much diminished. A gale is blowing from the west, and there has been an orange after-glow. Similar phenomena were described in your columns about this time last year. They were well seen in this part of the country. EDWARD GREENHOW

Earsdon, Newcastle-on-Tyne, December 28

YESTERDAY, clouds very similar to those seen a year ago made their appearance, and there were a few of them again this morning. I first noticed them at 11.30 a.m., and they were extremely magnificent after sunset, showing three or four spectra of colour, and they were especially striking about 4.10 p.m., when they appeared very bright against the purple glow of the sky. Their chief difference from the clouds last December was that they were not bounded by straight lines, and that there was no special amount of blue in the colouring, as was usually the case a year ago. The chief colours were pink and green.

Sunderland, December 29 T W. BACKHOUSE

Ventilation

IN reply to the query of J. F. Tennant, there can be no doubt that the cause of the failure of the ceiling ventilators is a deficiency of fresh-air supply to the room. An ordinary chimney with a fire will, if unchecked, draw an amount of cold air into the room which would make the temperature about the same as that of the outside air, and without enormous volumes of warmed air it is, I think, impossible to expect any service whatever from the system of ventilation from ceiling-flues, as recommended by the writer of the article referred to. Since writing my first letter I have seen a regenerator lamp attached to one of these ceiling-flues, and the down-draught was so strong and persistent as to reverse the natural current of the lamp, rendering its use impossible. The air-inlet to my own rooms consists of a channel in the wall of every room opening into ten one-inch holes at the fireplace, but this, of course, is utterly inadequate to supply one-tenth of the air required by the flue, and the ventilator and the ventilating-shaft supplement this supply by working the wrong way. THOS. FLETCHER

Warrington

A VERY common source of trouble with respect to ventilation is the absence of any arrangement for the supply of air to fires. So long as a fire draws on the general atmosphere of the room it is supposed to warm for its supply of oxygen, there must be the "draughts" so often complained of, and people are warm on the side next the fire, and cold on the other. I should suppose this is what happens in Mr. Fletcher's case, described in his letter in NATURE (pp. 153-4). If so, there is simply a sort of "tug-of-war" between the longer chimney-flue and the shorter ventilation flue, with the additional advantage on the side of the former that the column of air ascending the chimney is necessarily much warmer than that which should ascend the ventilation flue. If, however, Mr. Fletcher will have a couple of holes bored in his floor, one on each side of the fireplace, so as to supply air directly to the fire, the "pull" of the fire on the air of the room will cease, the room will be warmer, and his ventilation flue should work satisfactorily. I warm thoroughly a room with considerably over 2000 square feet of floor area by means of three small stoves. When first used the stoves were inefficient, as there was a draught all round each towards it. A common rain-water pipe "bend," inserted in the floor immediately in front of the aperture of each stove for admitting the air-supply, stopped the draughts, and at least doubled the efficiency of the stoves as warmers. With the help of Tobin tubes there is now a gentle current of warmer air *from* each stove. The heated and vitiated air escapes through ventilators fixed in the ridge of the roof. W. WILKINSON

Eldon, Bishop Auckland, December 23

Friction and Molecular Structure

IN your number of December 17 (p. 154) is a letter signed by Mr. E. Geoghegan, referring to the effect of moderate friction on the molecular structure of glass lamp-chimneys. This I have very frequently observed, and it would be very interesting to have suggestions as to its cause and means of prevention. I often read under one of Sugg's Argand gas-burners, the chimney of which almost invariably breaks on first heating after cleaning. First of all, washing was tried, to remove the mottled milky stain which forms on the glass, and then rubbing with a silk cloth or cotton rubber, but there does not seem to be much difference in the result, as the glasses, the best I can obtain, generally break. C. K. BUSHE

Bramhope, Old Charlton, Kent, December 25

The Longevity of Insects

WITH reference to the longevity of insects, it is worth while to record that we kept a ladybird from the September of one year to the September of the following. She was a handsome specimen of the seven-spotted ladybird, and her eggs, which were laid in the winter, after passing through the miniature crocodile stage, produced perfect insects in February. It is curious to watch the imago emerging from its dusky case; at first no spots are visible on its buttercup-yellow "shards," which contrast strongly with the jet-black legs and underneath; but in a very few hours the first brilliancy has gone, the spots appear faintly, and in a few days the final red with the black spots is established. E.

December 28

SOUTH AMERICAN BIRD-MUSIC

MR. BURROUGHS, an American naturalist, in his "Impressions of some British Song Birds," has said:—"Many of the American songsters are shy wood-birds, seldom seen or heard near the habitations of men, while nearly all the British birds are semi-domesticated, and sing in the garden and orchard. This fact, I had said, in connection with their more soft and plaintive voices, made our song-birds seem less to a foreign traveller than his own." These words apply with much greater force to the birds of South America, the species being much more numerous and less well known than in the northern portion of the continent; while the true songsters are relatively fewer, owing to the presence of several large songless families, such as the tyrants, humming-birds, and others.

The South American songsters certainly do not, like those of Europe, mass themselves about the habitations of men, to sing there as if sweet voices were given to them solely for the delectation of human listeners; they are pre-eminently birds of the wild forest, the marsh, and the savannah; and the ornithologist or collector from Europe, whose principal object is to make a large collection, has

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